

Comment on the proposed conservation of *Haliplanella* Hand, 1956 (Anthozoa, Actinaria) by suppression of *Haliplanella* Treadwell, 1943 (Polychaeta)
(Case 3493; see BZN 66: 312–316; 67: 166–167)

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We respond to den Hartog & Ates (BZN 67: 166–167) who commented on our appeal (BZN 66: 312–316) to resolve a homonymy by suppressing the name *Haliplanella* Treadwell, 1943 (for a polychaete annelid) in favour of the name *Haliplanella* Hand, 1956 (for an actinarian – a sea anemone). Most of the comments by den Hartog & Ates (BZN 67: 166–167) relate to taxonomy, not nomenclature. We reiterate our conviction that both taxonomy and nomenclature would best be served by the action we request.

We disagree with the assertion that ‘The introduction by Hand (1956) of the genus *Haliplanella* and of the family HALIPLANELLIDAE was exclusively based on the assumed presence of a combination of three types of nematocysts in the acontia’ (den Hartog & Ates, BZN 67: 166). Hand (1956), having observed three types of nematocysts from the acontia, illustrated them – he did not merely assume they were present. In addition, he included in the diagnosis of *Diadumene* the ability to develop catch tentacles (now commonly termed ‘fishing tentacles’), omitting mention of catch tentacles in the diagnosis of *Haliplanella*. Hand (1956) thereby implied that individuals belonging to the genus *Haliplanella* do not form catch tentacles, a distinction he explicitly stated (personal communication to DGF) as part of his conceptualising the genera (although we now know that that feature does not, in fact, differentiate them). The matter raised by den Hartog & Ates (BZN 67: 166) concerning the existence (or not) of a well-marked fosse and parapet was not considered by Hand (1956) and does not bear on this matter.

As is acknowledged by den Hartog & Ates (BZN 67: 166), assignment of the type species of *Haliplanella*, *Sagartia luciae* Verrill, 1898, to the genus *Diadumene* is not original to them (in a manuscript cited as in press). Indeed, we stated in our appeal (66: 313) that the animal that ‘is the most widespread species of anemone in the world’ has been ‘variously known as *Haliplanella luciae*, *H. lineata*, *Diadumene luciae* or *D. lineata*.’ An extensive list of the names used for this species, which is available online from Fautin (2009), includes several uses of the name *Haliplanella* during the past 2–3 years. This belies the assertion by den Hartog & Ates (BZN 67: 166) that ‘The recent proposal to conserve the name *Haliplanella* by Fautin et al. [BZN 66: 312–316] will serve no purpose.’ In fact, people continue to use the name, so resolving the homonymy would benefit the community.

As we pointed out (Fautin et al. BZN 66: 314), because the name *Haliplanella* Treadwell, 1943 is no longer used for the annelid, suppressing it will not cause hardship to any biologist. In addition to resolving a homonymy that exists irrespective of taxonomic considerations, placing on the Official List of Generic Names in Zoology the name *Haliplanella* Hand, 1956 will be a positive step; it will not put the name *Tricnidactis errans* de Oliveira Pires, 1987 in a nomenclaturally ambiguous situation pending taxonomic resolution of its affinities. That sea anemone was placed by its describer in the family HALIPLANELLIDAE. Although den Hartog & Ates, (BZN 67: 167) ‘think *T. errans* belongs to another family,’ they ‘have not been able to study this species.’ In discussing it, they raise taxonomic issues not directly germane to the nomenclatural basis of our appeal, including the philosophical position that ‘Species descriptions should not be based on a single isolated character’ (BZN 67: 167).

Additional reference

Fautin, D.G. 2009. Hexacorallians of the World. <http://geoportal.kgs.ku.edu/hexacoral/anemone2/index.cfm> (Accessed 22 July 2011)

Comment on the proposed conservation of usage of *Murex tubercularis* Montagu, 1803 (currently *Cerithiopsis tubercularis*; Mollusca, Gastropoda, CERITHIOPSIDAE) by designation of a neotype
(Case 3532; BZN 68: 41–46)

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We strongly support the application of Cecalupo and Robba and we fully agree with their well presented considerations.

Comment on the proposed conservation of *Termes serratus* Froggatt, 1898 and *Termes serrula* Desneux, 1904 (Insecta, Isoptera, TERMITINAE)
(Case 3385: see BZN 64: 83–86, 185–187; 65: 47–49, 132–136; 66: 342–348)

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I’ve read Jones’s proposal and all the subsequent comments. I realise the decision of Roisin & Pasteels (2000) was strictly correct. However as a taxonomic researcher of termites in Southeast Asia, I wish to conserve the scientific name *Microcerotermes serratus* (Froggatt) as an Australian species and *Microcerotermes serrula* (Desneux)

as a Southeast Asian species. From my fieldwork I know that *M. serrula* (Desneux) is common and widespread across Sundaland. Also, the soldiers have very distinctive characters and are easily identified using the standard texts (Thapa, 1981 and Tho, 1992). As a consequence, I have seen many series of this species labeled as *M. serrula* in termite collections in museums, government forestry departments and universities in Malaysia, and the same is probably true of Indonesian institutions in Kalimantan, Sumatra and Java. It is clear that many people including non-termitologist researchers and government officers have used the name *M. serrula* (Desneux) for the Southeast Asian species. Evans (BZN 66: 343) was correct when he mentioned that this name had been used not only in scientific publications but also in multiple government reports about forestry and biodiversity. If the name is not conserved, then to avoid confusion a large number of specimens deposited in various institutions would have to be re-labeled as *M. serratus*, or people who access these specimens would have to be aware of the name change. I therefore support the views of Jones (BZN 64: 83–86) and Evans (BZN 66: 342–346).

Comments on the proposed establishment of availability of *Balintus* d’Abrera, 2001, *Gulliveria* d’Abrera & Bálint, 2001, *Salazaria* d’Abrera & Bálint, 2001, *Megathecla* Robbins, 2002 and *Gullicaena* Bálint, 2002 (Insecta, Lepidoptera, LYCAENIDAE) (Case 3458; see BZN 65: 188–193, 66: 271–272, 66: 349–351)

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1. We are convinced that our original descriptions of taxa described in d’Abrera (2001) have been misinterpreted by the applicants in Case 3458. All the eight generic names we proposed, but which were questioned by the applicants, have been correctly established in a proper and scientific manner, and thus they have incontrovertibly become available for zoological nomenclature. Our wording closely adheres to the Articles of the Code.

2. The descriptions of all the genera proposed by us in d’Abrera (2001) appeared under appropriate headings, each having the newly proposed generic names in bold capital letters with bold typeface and indicated as ‘gen. nov.’. The first entry after each heading clearly established the particular ‘Type Species’ for the proposed genus, stating (as required by the Code) the name of the selected taxon in its original combination. The next paragraph in logical sequence listed those morphological characters that we have used to diagnose the proposed genus through its selected type species.

3. Contrary to the claim of the applicants, this action is not ambiguous. In providing a description of the characters existing on the type species as the standard representative of the proposed genus, we are strictly following the requirements of

the Code. We have already pre-indicated in the heading in bold capital letters the putative taxon to be diagnosed. Thus it is a clear statement that the subject is the genus novum and not the type species.

4. The final entry in the text, with the unambiguous subheading 'Congeners' further places all the taxa we considered to be encompassed by the new genus in their new combinations with the bold typeface indicating 'comb. nov.', which means that this is a new combination of specific and generic epithets, the species being well established, but the genus now being established as a nomenclatural novelty in combination with it.

5. Although we think it is unnecessary to have to explain our precise wording, we now do this to balance the inaccurate explanations of the applicants who have brought Case 3458 for their own purposes. We use the most discussed genus *Annamaria* as an example.

6. Article 13.1.1 states that a name is available when it is accompanied by a description or definition that states in words characters that are purported to differentiate the taxon. The heading of our nomenclatural (proposing a new name) and taxonomic (to describe and to define a new taxon) action clearly states the sole subject of the paragraph: that is 'GENUS *ANNAMARIA* d'Abrera & Balint **gen. nov.**' the name and the taxon, which is to be established and differentiated in the subsequent entry. It is accompanied by a description stating in words the use of certain characters of the type species, which is the objective standard of the genus. Therefore there is (a) a new genus-group name proposed, (b) a description which states in words that these are characters of the type species (the objective standard of the genus), which define the taxon (the new genus) and (c) help to differentiate the taxon from the previously described ones.

7. The applicants have artificially constructed a situation in which the proposed name *Annamaria* is a nomen nudum, because (they imply) there is no description or definition of the name itself, thereby rendering it unavailable for any nomenclatural action. If the sole subject of our taxonomic and nomenclatural action was to be simply the type species *in vacuo*, then the heading 'GENUS *ANNAMARIA* d'Abrera & Balint **gen. nov.**' would be nonsense. Hence, to render the name unavailable the applicants coined the phrase 'implied grammatical subject' for our sentences, which they chose to interpret accordingly. This distorted an otherwise straightforward nomenclatural and taxonomic action and obscured what we originally presented, which was a correct, comprehensive but economic taxonomic description of the genus *Annamaria* gen. nov.

8. By using such a distortion it becomes only too easy to question not only the availability of the names we proposed, but many previously established names by other authors. For instance, the genera *Famegana* Eliot, 1973, *Rysops* Eliot, 1973, *Titea* Eliot, 1973, and *Zintha* Eliot, 1973, taken from one of the most fundamental works ever published on LYCAENIDAE classification (Eliot, 1973), would also be rendered unavailable as well. The descriptions of the mentioned names established by Eliot were worded in an almost identical manner to the questioned generic names published in d'Abrera (2001); but interestingly none of these names have been considered to be unavailable by the applicants (Lamas, 2008a).

9. We maintain that our 'interpretation' is sound and in logical union with the heading, the meaning and the intention of the text. It is only ambiguous in the eyes

of the composers of the case; there are no semantic grounds to justify their 'implied grammatical subject'. The meaning of d'Abrera & Bálint is given expressly in the heading: 'In NEOTROPICAL VII:1107 [GENUS *ANNAMARIA* d'Abrera & Bálint **gen. nov.** was] treated as *Evenus draudti* [with congeners]. Likewise by other workers. However [GENUS *ANNAMARIA* d'Abrera & Bálint **gen. nov.**] is distinguished from *Evenus* by shorter cell of f.w. (1/3rd of costal length), and extension of Vein 1 of h.w. into a lobed tail at tornus. [GENUS *ANNAMARIA* d'Abrera & Bálint **gen. nov.** has a] compound androconial patch on male f.w. consisting of single circle within cell & quadrifurcate patch immediately outside discocellulars. Further, [GENUS *ANNAMARIA* d'Abrera & Bálint **gen. nov.** has] androconial patches on post discal & submarginal tornal areas of f.w. respectively.'

10. In the Code there is nothing to indicate that the characters of the selected type species could be regarded as anything but characteristic of the genus which it was deliberately chosen to represent. Nor does the Code indicate that describing other characters only present in the congeners would somehow be necessary to make the proposed name available from a nomenclatural point of view. Moreover, we make an historical note that our concept of *Annamaria* (in d'Abrera, 2001; Bálint, 2005) was still partly divergent from that of Robbins' *Lamasina* (Robbins, 2004b), but later the applicants came to similar conclusions (Robbins & Lamas, 2008). This objectively demonstrates that the original definition of the genus *Annamaria* was sound in spite of the criticism of the applicants.

11. Therefore we maintain that the establishment of all of our new genera could not have been composed in a more straightforward way and that we did this in accordance with the Code. Interested readers can check all of our claims in the pages of the *Concise Atlas of the Butterflies of the World* (d'Abrera, 2001; there is a generic index), or consult the Fig. 3. of Bálint (2005), which is a facsimile of the original description of the genus *Annamaria* d'Abrera & Bálint, 2001 and compare our style and wording with those of Eliot (1973) and judge for themselves whether there are objective grounds or genuine need by the applicants for bringing such a case in the first place.

12. Expressing the need to correct some nomenclature for a yet-to-be-published manuscript, one of the applicants (Robbins, 2002) preferred the forgotten, homonymous and therefore unavailable name *Eucharía* Boisduval, 1870, which was briefly and inadequately described. Moreover, the type species for that genus was only subsequently designated through a corrective nomenclatural procedure. The name *Annamaria* was published with a designation of a type species and clearly listed generic characters, and is therefore preferable. Hence, *Annamaria* should at least have been cited objectively by the applicants: consequently it must be applied. In contrast, for the unavailable name *Eucharía*, one of the applicants proposed *Lamasina*, although he had clearly demonstrated that he was well aware of the existence of the name *Annamaria*, a senior synonym (Robbins, 2002). This applicant failed to use the name in such manner, instead considering it subjectively unavailable (Robbins, 2004; Robbins & Lamas, 2008; see Bálint, 2009, 2010).

13. The same applicant correctly replaced the junior homonym *Gulliveria* d'Abrera & Bálint, 2001 with *Megathecla* Robbins, 2002 but that applicant made no attempt to contact either of us to inform us that the homonym *Gulliveria* was in need of replacement, as recommended by the Code of Ethics. The other names in the case

were not considered to be unavailable at that time, but this concept has since been reversed in the eumaine checklist compiled by one of the applicants (Robbins, 2004b) for a book edited by the other applicant (Lamas, 2004). This was finally crystallised in Case 3458 and subsequent joint papers of the applicants (see Bálint, 2009).

14. The applicant working on a checklist of neotropical butterflies did contact one of us (Dr Bálint) in early 2002 when the nomenclatural note written by his colleague (the other applicant) had most probably already been printed, or was in the last stages of preparation for press. It was only at that late moment that the applicant first drew the attention of Bálint to *Gulliveria* being homonymous, but did not mention that his colleague's paper (Robbins, 2002) had already been submitted. This again appears counter to the ethical recommendations in the Code. Meanwhile, Bálint's (2002) paper with the replacement name *Gullicaena* Bálint was written, submitted, accepted and published on November 30 2002, with no knowledge of the other submission on the subject. Therefore the publication date of *Megathecla* Robbins (26 June 2002) indeed preceded that of the replacement name *Gullicaena* Bálint, 2002 by five months, though we maintain this was not executed in a manner concordant with the ethical recommendations of the Code.

15. We feel this lack of communication was indicative of an uncooperative attitude among workers on Neotropical eumaine lycaenids, expressed in a paper authored by one of the applicants (Robbins, 2004a) and published in a book edited by the other applicant (Lamas, 2004). In the application to the Commission, the taxonomic descriptions in d'Abrera's (2001) book (which was the first modern taxonomic overview of Neotropical eumaines) are, in our opinion, misinterpreted and the availability of new names is incorrectly questioned.

16. One applicant recently published a paper in which he proposed twelve species-group replacement names in the family LYCAENIDAE as part of his work on butterfly nomenclature on a global scale (Lamas, 2008b). Appropriately, most of his new names honour the authors of the junior homonyms or the collector, or refer to the geographic localities of the taxa. However, two homonyms, namely *Plebejus* (*Plebejides*) *pylaon forsteri* Bálint, 1990, and *Albulina tibetana* d'Abrera, 1993, received the replacement names that cause us to question if this was an inappropriate test of the boundaries of point 4 of the Code of Ethics ('no author shall propose a name . . .that would be likely to give offence on any grounds'). The name proposed to replace *forsteri* is *tumultus* (confusion), while the name proposed to replace *tibetana* is *chaos* (disorder). We respectfully suggest that it was only through publishing Case 3458 that the authors have created tumult and confusion for these names themselves. We also underscore that, had the authors adhered to point 3 of the Code of Ethics, we, as authors of these names, would have been happy to establish appropriate substitute names.

17. If this application were to be upheld, the Commission would permit a destabilising situation in which any nomenclaturist or taxonomist might feel justified in attempting to dismiss other publications with no objective justification, and would undermine the need to apply to the plenary powers of the Commission to suppress many names. Any paragraph with economic wording purporting to describe taxa would be at risk. The sense or meaning of a taxon would be at the mercy of manipulative grammatical or syntactic interpretation, determined by the agenda of

the revisor, who could apply his subjective interpretation of the ‘purpose’ of any original text.

18. We have concerns that one of the authors of this Case is also in a position to vote on its outcome. We request that the Commission consider the ethical justification if this vote is a deciding factor in the outcome of the Case.

19. In short, Case 3458, if upheld by the Commission, would create tumult and chaos and undermine the main brief of the Code, which is the stability of scientific nomenclature.

Additional references

- Bálint, Zs.** 2009. Five chapters on *Annamaria columbia* with the description of a new genus (Lepidoptera: Lycaenidae: Eumaeini). *Boletín Científico Centro de Museos, Museo Historia Natural, Caldas*, **13**: 75–82.
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- Robbins, R.K.** 2004b. Lycaenidae. Theclinae. Tribe Eumaeini, pp. 118–137 in Lamas, G. (Ed.), Checklist: Part 4A. Hesperioidea – Papilionoidea. In Heppner, J.B. (Ed.), *Atlas of Neotropical Lepidoptera*, vol. 5A. 439 pp. Association for Tropical Lepidoptera; Scientific Publishers, Gainesville, Florida.

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This proposal should be rejected for the following reasons:

Any description of a new genus which contains the words ‘is differentiated by’ or an equivalent phrase, followed by a series of anatomical characters must be construed as purporting to differentiate the genus. Article 13.1.1 of the Code only requires that an author states characters that are purported to differentiate the taxon, not that the characters are diagnostic, nor that the differentiation is perfect, as long as the statement is purported to do so. It is not for Robbins or Lamas to decide what d’Abrera & Bálint (in d’Abrera, 2001) purported, any more than it is appropriate for others to draw conclusions about Robbins & Lamas’s motives in lodging this application. It is common sense to assume that d’Abrera & Bálint purported to do what they were required to do, i.e. differentiate their new genera.

Although the wording of the description of *Annamaria* (and the implication of its one missing word) has often been quoted as an example in this debate, the other seven new genera proposed by d’Abrera & Bálint (in d’Abrera, 2001) were each introduced

using slightly different wording. For this reason these names can not be rejected in bulk, as requested in this application. Each one should be examined on its own merits and ruled on individually.

Robbins & Lamas (BZN 66: 349–351) have repeatedly claimed that this action is being taken only in the interests of stability and universality, which d’Abrera & Bálint (in d’Abrera, 2001) apparently ‘breached’ in some way by describing eight new genera, that nobody has yet questioned with regard to the soundness of the concepts involved. There was no instability or confusion in 2001 when these were described. All the problems that have ensued were initiated by Robbins (2002) when he unilaterally declared the name *Annamaria* unavailable. The only confusion now is whether this genus should be known by its senior name of *Annamaria* d’Abrera & Bálint (in d’Abrera, 2001) or by its later subjective synonym *Lamasina* Robbins, 2002.

For reasons that the applicants have never fully explained, the Commission is being asked to make five (supposedly unavailable) names available, two of which are to be immediately suppressed and thus made unavailable, along with six others, five of which have barely been discussed in the application and appear to represent taxa for which there would then be no alternative generic names. The two names proposed by Robbins (2002) are conspicuous among the four survivors of this nomenclatural massacre. The rejection of this proposal would confirm that all eight generic names are available from their original descriptions.

**Comment on the proposed designation of a neotype for the nominal species
Chionobas chryxus Doubleday, 1849 (currently *Oeneis chryxus*; Insecta,
Lepidoptera, NYMPHALIDAE)
(Case 3495; see BZN 67: 121–128; 68: 136–140)**

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There is clear evidence that there are two separate species throughout the Rocky Mountains. I personally collected *altacordillera* in Alberta at Nigel Pass and Highwood Pass. Charles Harp and Steve Kohler collected numerous *altacordillera* in Montana, the former’s specimens now in the University of Colorado museum. Specimens in that museum show both species fly together in the Wind River Mts. of Wyoming. Paul M. Thompson and David Threatful collected *altacordillera* at Gott Peak in British Columbia, and Norbert Kondla found it on Mt. Spieker. In NE Nevada *altacordillera* occurs in the Snake Range, while the other species has been found in the Egan Range. The two species are sympatric at 20 known locations throughout the Rocky Mountains. And I recently (Scott, 2008) found that larvae of *altacordillera* (including two subspecies from Washington and Ontario) have a different coloration and usually have a dashed heart-band on larvae, compared to the species depicted in the original *chryxus* painting which has a solid heart-band. No known butterfly has subspecies with oviposition behaviour as different as these two *Oeneis* taxa have. The problem here is that it takes time for people to learn how to

identify new similar taxa, and not all lepidopterists have acquired those skills. Difficulty of identification is nothing new: four species of *Phyciodes* (NYMPHALIDAE) and seven of *Celastrina* (LYCAENIDAE) are now known in eastern United States, up from two and one a few decades ago, and most lepidopterists still cannot identify those. Females are not as good as males for identification in *Oeneis chryxus*-group species: *O. nevadensis* and *O. macouni* females are almost identical while the males are very different, and *O. alberta* females often resemble *O. 'chryxus'* females.

The ICZN governs nomenclature, not taxa, so squabbling about limits of taxa is largely irrelevant. It is enough to state that the people who have carefully studied these taxa think there are several taxa in Alberta, and a neotype is needed because the lectotype has dubious taxon identity and disputable locality.

I had thought that the proposed *chryxus* neotype would be acceptable to other lepidopterists, as it comes from the Alberta location that people generally cite as the type locality, and it matches the phenotype of the original painting. Surely it is preferable to stabilise nomenclature before a large body of literature using confused names accumulates, rather than after. However, from a biological viewpoint, the optimal neotype should come from an area where the biology of both species has been well studied (Colorado), and the biology of these butterflies is little known in Alberta. I was informed that the Commission can render an Opinion on this case in multiple ways, so to satisfy the doubters and permit an optimal neotype, the best way would be for the Commission not to designate a neotype, and either merely affirm that the wording of Articles 72.4.1 and 72.4.1.1 of the Code is not a mistake, or make no decision at all on the case. As written, Article 72.4.1.1 is numbered and indented as subservient to 72.4.1; therefore 72.4.1.1 allows one to consider specimens not mentioned in the original publication as part of the type series, only if those specimens belong to the taxon defined by the original publication. This interpretation prevents the worst calamity that can befall a lectotype (a lectotype that proves to belong to a taxon different from that defined in the original publication), therefore the writing in the 4th edition of the Code is a considerable improvement over the 3rd edition. When this case was reviewed prior to publication, two Commissioners agreed with this restrictive interpretation of Articles 72.4.1 and 72.4.1.1, which with Article 86.3 invalidates the lectotype, and wrote that I could just designate a neotype without petitioning the Commission. So the absence of an Opinion on this case would satisfy doubters and would permit an optimal neotype, although a statement that the wording is not a mistake would contribute to John Calhoun's request for clarification of this Article.

Even if the original male *chryxus* were found, it would be considered merely a useless paralectotype by anyone who thinks the lectotype is valid. Also, Article 73.1 clearly confirms that the male illustrated in the original publication is the holotype, and Article 73.1.2 states that evidence outside the work may be taken into account to help identify that specimen – any other conspecific specimens found would be paratypes, not syntypes.

Additional reference

Scott, J.A. 2008. Early stages of *Oeneis calais altacordillera* Scott (plate V). Pp. 25–29, pl. 5 and pl. 5 continued, in Scott, J. & Fisher, M.S. Geographic variation and new taxa of western North American butterflies, especially from Colorado. *Papilio* (New Series), **18**:1–72.

Comments on *Stegosaurus* Marsh, 1877 (Dinosauria, Ornithischia): proposed replacement of the type species with *Stegosaurus stenops* Marsh, 1887 (Case 3536; see BZN 68: 127–133)

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In Case 3536, Galton outlined the taxonomic history of the iconic dinosaur genus *Stegosaurus*. In this Case, Galton asked the Commission to designate *Stegosaurus stenops* as type species of the genus *Stegosaurus*, thereby allowing the holotype specimen of *Stegosaurus stenops*, USNM 4934, to become the representative of the genus *Stegosaurus*.

The Case is complicated by the fact that those who have worked on the taxonomy of *Stegosaurus* do not agree about the taxonomic validity of various genera and species, as clearly outlined by Galton. In Case 3536 Galton suggested that the type specimen of *Stegosaurus armatus* (YPM 1850), which is the type species of *Stegosaurus*, bears no synapomorphies of *Stegosaurus* or autapomorphies of its own, making the name *Stegosaurus armatus* a nomen dubium. However, Mossbrucker et al. (2009) have suggested that YPM 1850 may bear an autapomorphy, making the name *Stegosaurus armatus* valid.

If YPM 1850 is undiagnostic, the generic name *Stegosaurus* is a nomen dubium. If YPM 1850 is diagnostic, as has been tentatively suggested by Mossbrucker et al. (2009), the name *Stegosaurus armatus* would likely be restricted to YPM 1850 because, as argued by Galton in the Case, YPM 1850 bears no other synapomorphies of *Stegosaurus* (in its current usage); thus all other material currently referred to the genus *Stegosaurus* would need a new generic name. *Hypsirhophus discursus* was named by Cope (1878) for a partial dorsal vertebra (AMNH 5731). Galton (2010) considered this specimen to be diagnostic and *Hypsirhophus* a distinct genus although for Maidment et al. (2008) and Maidment (2010) *Hypsirhophus* is the next available nominal genus to contain all other species of stegosaur formerly included in *Stegosaurus*.

Stegosaurus is one of the most iconic and most recognisable dinosaurs to both the public and scientists alike; the loss of the name *Stegosaurus* is therefore an unfavourable outcome.

Maidment et al. (2008) suggested that all stegosaur material from the Morrison Formation of the USA belonged to a single species (except for material described as *Hesperosaurus mjosi* by Carpenter et al. [2001]). Maidment et al. (2008) named this species *Stegosaurus armatus*, but diagnostic characters were based on a referred specimen, USNM 4934, the holotype of *Stegosaurus stenops*, which Maidment et al. (2008) considered to be a junior synonym of *Stegosaurus armatus*. Designating *Stegosaurus stenops* as the type species of *Stegosaurus* results in USNM 4934 being the specimen on which *Stegosaurus* is based. This is entirely appropriate because USNM 4934 is one of the most complete stegosaurs known from anywhere in the world, and the specimen has been used as the reference specimen against which other stegosaurs are compared since a detailed and definitive description of it was

published (Gilmore, 1914). This is entirely in keeping with the work of Maidment et al. (2008), because USNM 4934 was used as the reference specimen in that work.

As Galton has argued in the Case, it is more favourable to designate *Stegosaurus stenops* as the type species of *Stegosaurus* than to make USNM 4934 the type specimen of *Stegosaurus armatus*, because of the questions surrounding the presence or absence of diagnostic characters in the holotype of *Stegosaurus armatus*. By designating a new type species for *Stegosaurus*, problems of taxonomy relating to YPM 1850 are circumvented. I therefore fully support the proposal by Galton in Case 3536.

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The taxon *Stegosaurus armatus* was established by O.C. Marsh in 1877 on a very fragmentary specimen from the Morrison Formation near Morrison, Colorado (erroneously stated to be 'Morrison, Wyoming' by Galton, BZN 68: 127). The specimen was encased in silicified sandstone and collected very poorly by modern standards using hammers and chisels, plus explosives to reduce the rock into more manageable pieces. The result is that much of the specimen was greatly damaged and many pieces missing, thus making it only marginally diagnostic (Carpenter & Galton, 2001), as noted by Galton (BZN 68: 130) in his petition. Such situations are unfortunately common for dinosaur specimens named during the 1800s that now require petitions to the Commission to ensure their stability (e.g. Case 3037, Charig & Chapman, 1998; Case 3506, Paul & Carpenter, 2010). In these examples, specimens displayed characters once thought to be unique but which were later found to be more widely distributed through the discovery of more complete specimens. Wilson & Upchurch (2003) refer to this as 'historical obsolescence'. *Stegosaurus armatus* certainly falls into this category in that the hexangular caudal vertebrae and large, plate-like osteoderms were thought unique among the Dinosauria. However, subsequent discoveries in Africa, Asia, Europe, and North America have shown that these characters occur in other taxa referred to the Stegosauria. As noted by Galton (BZN 68: 131), the type of *S. armatus* has no autapomorphic characters, therefore it cannot be separated from any other taxon of Stegosauria.

In contrast to *S. armatus*, the nominal species *Stegosaurus stenops* Marsh, 1887 is represented by several nearly complete skeletons and thus is very well known. These specimens form the basis for the current concept of the genus *Stegosaurus* (Marsh, 1887, 1891; Gilmore, 1914; Carpenter & Galton, 2001; Carpenter et al., 2001; Galton & Upchurch, 2004; Maidment et al., 2008; Carpenter, 2010; Galton, 2010). Because *Stegosaurus* is such an iconic dinosaur, and because the name is so well entrenched in the scientific literature, its name should be associated with material of taxonomic utility. That such is not currently the case is shown by Maidment et al. (2008) declaring *Hypsirophus discursus*, *Stegosaurus ungulatus*, *S. duplex*, *Diracodon laticeps*, and *Stegosaurus stenops* to be junior synonyms of *S. armatus*. However, the result is the creation of a 'superspecies' showing a wider range of non-ontogenetic variation throughout the skeleton than any other species of Dinosauria, except waste-basket

taxa (e.g., *Iguanodon* prior to Paul, 2008). As Carpenter (2010) has noted, the range of variation in *S. armatus* (sensu Maidment et al., 2008) cannot be replicated in other large samples of stegosaurids (e.g. *Kentrosaurus aethiopicus* from Africa), therefore casting doubt on the validity of the variations, which in turn casts doubt on the concept of *S. armatus* as defined by Maidment et al. (2008). All of this confusion would be eliminated by replacing the nominal species *S. armatus* with *S. stenops* as petitioned by Galton (BZN 68: 127–133), thereby ensuring taxonomic stability for the well-known genus *Stegosaurus*.

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I am writing in support of the petition (Case 3536) by Galton to replace *Stegosaurus armatus* Marsh, 1877 with *S. stenops* Marsh, 1887 as the type species of *Stegosaurus* Marsh, 1877.

Maidment et al. (2008) diagnosed *Stegosaurus* on the basis of the following autapomorphies: (1) Quadrate-squamosal-paroccipital process articulation overhangs the retroarticular process of the lower jaw; (2) postzygapophyses on posterior cervical vertebrae are elongated posteriorly and overhang the back of the centrum; (3) transverse processes on anterior caudal vertebrae (except for caudals one and two) project ventrally rather than laterally; (4) large, rectangular acromial process of the scapula; (5) supra-acetabular process diverges at an angle of 90 degrees from the anterior process of the ilium; and (6) medial process present on the posterior iliac process of the ilium. They also noted that *Stegosaurus armatus* (= *Stegosaurus* sensu Carpenter et al. 2001 of my usage) differs from all other stegosaurs in having: (1) edentulous portion of the dentary anterior to the tooth row and posterior to the prementary; (2) dorsally elevated postzygapophyses of the cervical vertebrae; (3) bifurcated summits of the neural spines of the anterior and middle caudal vertebrae; (4) unexpanded posterior end of the pubis; and (5) dermal ossicles embedded in the skin on the underside of the cervical region. They referred all stegosaur taxa from the Morrison Formation (except *Stegosaurus sulcatus*, *S. longispinus*, and *Hesperosaurus mjosi*) to *S. armatus*.

Of the autapomorphies cited for STEGOSAURINAE (= *Stegosaurus*) and *Stegosaurus* (= *S. armatus*) by Maidment et al., only two characters can be observed in the holotype of *Stegosaurus armatus* (YPM 1850): transverse processes on anterior caudal vertebrae (except for caudals one and two) project ventrally rather than laterally and bifurcated summits of the neural spines of the anterior and middle caudal vertebrae. As acknowledged by Galton (2010), the presence of transverse processes on anterior caudal vertebrae (except for caudals one and two) that project ventrally rather than laterally is not confined to YPM 1850 and other specimens referred to *S. armatus* by Maidment et al. (e.g. USNM 4934, YPM 1853) but is also found in *Hesperosaurus mjosi* and *Stegosaurus longispinus*. The caudals of YPM 1850 exhibit bifurcated summits of the neural spines of the anterior and middle caudal vertebrae (Carpenter & Galton, 2001, fig. 4.4G; Galton, 2010, fig. 1b), an autapomorphy of *Stegosaurus armatus* according to Maidment et al., but as Galton demonstrated, this character is also present in *Stegosaurus ungulatus* (YPM 1853, YPM 1858), *S. stenops* (USNM 4934, DMNS 2818), *S. longispinus* (UW 20503), and the holotype of *Hypsirophus discursus* (AMNH 5731). Using the updated list of synapomorphies for Stegosauria, and STEGOSAURIDAE provided by Mateus et al. (2009, supplementary information), a stegosaurian placement of *S. armatus* is supported by the presence of two parasagittal rows of plates or spines extending from the cervical region to the end of the tail (Carpenter & Galton, 2001, fig. 4.5C). YPM 1850 can be assigned to STEGOSAURIDAE based on the presence of a dorsal process on transverse process of caudal vertebrae and anterior caudal vertebrae with bulbous swellings at the top of neural spines (Carpenter & Galton, 2001, figs 4.4D, F).

Using the criteria outlined by Galton regarding the autapomorphic structure of dermal armor for Morrison stegosaur species, *Stegosaurus ungulatus*, *S. stenops*, *S. longispinus*, and *Hesperosaurus mjosi* differ from each other in the form of the dermal armor, as well as characters of the femur and ilium, as noted by Galton. However, except for fragments of a large dermal plate, no dermal armor is preserved in the holotype of *S. armatus*, so YPM 1850 lacks any dermal characters that would distinguish it from *S. ungulatus*, *S. stenops*, *S. longispinus*, or *Hesperosaurus mjosi*.

In a recent abstract, Mossbrucker et al. (2009) indicated that the holotype of *Stegosaurus armatus* is distinguishable from other Morrison stegosaurs by the presence of unusually robust neural spines, based on recent preparation of the holotype at the Morrison Natural History Museum (MNHM). However, this character is likely to be a product of individual variation within a species, and the results of Mossbrucker et al. have not yet been published. Thus, STEGOSAURINAE (= *Stegosaurus* sensu Maidment et al., 2008) comprises three valid genera, *Hesperosaurus*, *Stegosaurus*, and *Wuerhosaurus*; *Stegosaurus* sensu Carpenter et al., 2001 (= *Stegosaurus armatus* sensu Maidment et al., 2008) comprises three valid species (*Stegosaurus ungulatus*, *S. stenops*, and *S. longispinus*), with *Stegosaurus armatus*, *Hypsirophus discursus*, *Diracodon laticeps*, and *Stegosaurus sulcatus* referable to *Stegosaurus* sensu stricto (restricted to *S. stenops*, *S. longispinus*, and *S. ungulatus*) as nomina dubia. I provisionally agree with Galton in considering *S. armatus* a nomen dubium and restricting it to YPM 1850 until the results of Mossbrucker et al. are published and YPM 1850 is fully described.

Additional references

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